Independent Review of the Scientific Management Recommendations in the June 1998 Large Coastal Shark Evaluation Workshop Report

Reviewer No. 5[1]

Received on October 2, 2001

Natural Resources Consultants, Inc.

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[1] Reviewers' statements are provided in their original and unaltered form. The name of the reviewer is available upon written request to NRC with the approval of the court.

Statement: I have reviewed the June 1998 Large Coastal Shark Evaluation Workshop Report and the other documents submitted by the Court for review and have come to the following conclusions regarding the scientific management recommendations contained in the 1998 SEW Report.

Court Directed Question:
Response in respect to the Court requirement that: .....

Response:
The question has the following two parts:
[1] the 1998 SEW Report is based on scientifically reasonable uses of the appropriate fisheries stock assessment techniques and [2] the best available biological and fishery information related to large coastal sharks.
The answer to [1] is "no" and the answer to [2] is "yes". Much of the content of the 1998 SEW report was developed in response to the contents of earlier SEW reports which concluded that the large pelagic stocks would not recover to MSY levels for decades, even under the proposed lowered TACs. I believe that the production model was included in the 1998 SEW to provide a firm scientific basis for that conclusion. The production method did succeed in reaching the same conclusion but, its value is negligible since the model is inappropriate and should not have been used.

Question 1. *Was the model used to estimate large coastal shark population abundances and demographic trends reliable and scientifically rigorous?*

Response: Technically, this question is slightly different than the preceding one. The production model is not an appropriate model for the estimation of anything that is demographic in nature because of the absence of an age-structure component. The production model is inappropriate for the species – group to which it was applied. It was not reliable for several reasons, including that: the model assumes a steady state, a closed population, and density dependent processes. Furthermore, a symmetric, parabolic model was assumed valid. Scientists know that populations that are highly age –structured are not well described by production models. The population dynamics of most sharks are highly age structured. The strength of production models is precisely that, in the absence of biological detail [life history data], it is possible to use the logistic model as the implicit foundation to compensate for the lack of biological knowledge.

Question 2. *Were the scientific conclusions and scientific management recommendations based on a logical extension of the model's results?*

Response: Given that the model was inappropriate, conclusions reached based on the model cannot be justified. Other models do exist that would be much more appropriate. An alternative would be to introduce statistical corrections to those assumptions of the model that are not justified.

The production model was not the entire contents of the 1998 SEW. The report rests upon SEWs from previous years which also drew the conclusion that the TACs for large pelagic sharks were not sustainable and had to be reduced to bring the harvests to sustainable levels. Other, additional life history analyses were also included in the 1998 SEW that support reductions in the TACs for several of the large pelagic sharks. Whereas the production model is one part of the 1998 SEW and is not defensible, the parts that also support reduced TACs remain valid.

Responses to Directive:

Question 1. *how the stock assessment applied the Bayesian modeling approach to the available data and determined the appropriateness of using a non-age specific production model to assess a long lived species or complex.*

Response: Mathematicians always face the need to balance the estimation of parameters against the complexity of large models, with large numbers of parameters. The simplicity of the production model is intellectually attractive precisely because of the ease of carrying out maximum likelihood parameter estimation. In brief, if one wants answers keep the model simple. Given the range of alternative models available, the authors made a poor choice. But, as suggested, the simple model made use of a Bayesian structure for estimation very practical. As far as can be determined without repeating all of the analyses, it appears that the estimation part of the effort was expertly carried out. Other issues about the fitting are important for a full understanding of the estimation. These are explored in the mathematical analyses I did, which are described in the first comment at the end of this report.

Question 2: *how the stock assessment considered the availability and quality [how the series were estimated, how they were weighted for analyses and how they were applied as age specific indices of abundance, particularly for the MRFSS data which accounts for most of the LCS mortality in the early years] of available data sets..*

Response: Available data is sparse and with relatively short time series. The NMFS should have been collecting these data over a longer period of time and when they did begin to direct effort at data collection it appears that they did not receive as much cooperation from industry as they might have. Age specific data is basically not available since realistic age-length keys do not exist and cannot be developed quickly. If the above question means "cpue" when it says "indices of abundance", it should be noted that it is difficult to decompose cpue from catch to an age specific category and may be meaningless when done, depending on the species. If the industry would agree to minimum size restrictions on catch then size specific cpue data would de facto be available. I have not carefully examined the VIMS data base and carried out an exploratory data analysis. Certainly methodology needs to be standardized over years.

Question 3: *how the stock assessment handled and applied information relating to whether the species of LCS under consideration represent open or closed populations in individual instance?*

Response: the assumption that the system is closed is an important part of most model applications. NMFS has a lot of experience correcting for violations of the assumption. The difficulty here is that only ad hoc corrections have been used. It is quite possible to add a biomass additions term to the Schaeffer Stock Assessment model but a closed form solution would not result. Short of re-doing the analysis with another model, NMFS has probably done all that can be done with this assumption.

Question 4: *how the stock assessment evaluated the reliability of projections based on the above three considerations?*

Response: the stock assessment that industry calls the WCS model essentially supported limitations on the fishery that were arrived out via less mathematically sophisticated methods. The WCS model is unfortunate, but the remainder of the report and analyses should not be condemned because of the WCS model's shortcomings.

Question 5: *how the stock assessment evaluated the effects of extant regulations on stock trajectories and weighted the risk of maintaining the status quo until these effects could be evaluated against the costs of an additional immediate reduction in permitted LCS landing levels?*

Response: there is no doubt in my mind that the NMFS was obligated to further restrict fishing regulations on the LCS species based upon the several different data bases available to them. This would be true even if the obligation to carry out "precautionary" management did not exist. This is said entirely independently of the results of the WCS model. I feel that size, location and season limitations may have been preferable to the level of the imposed quotas, but that could only be established after careful analysis.

COMMENTS

Comment 1:
The CPUE data are the only catch data available and are characterized by being of relatively short time series and are rather variable. Given that the choice of a production model was connected with the CPUE data in-hand, and that the model itself is questionable, at best, there is little reason to put tremendous effort into the parameter estimation techniques. Nevertheless, there are good reasons to explore the estimation process in a broad sense.

In the following we used the same deterministic model used by McAllister and Pikitch [1998] (M-P) taken from Prager [1994], equation 4a. We used only CPUE12 and

CPUE13 data and maximum likelihood estimation of two parameters simultaneously: r, for the intrinsic rate of increase and K, for the carrying capacity. Our two figures [attached] correspond to suites of [r,K] values from the likelihood profiles where the objective function is satisfied and is less than some fixed number. They correspond to the Figures 1 a] and b] in M-K [1998]. The darkest areas in our figures correspond to values of [r,K] that produce mean square values < 1.0 in the objective function in Prager [1994], equation under A3'. The darkest areas in M-P [1994] do not appear to be specified. In the following, we assume that the black areas in M-P and in ours are comparable.

Considering the darkest areas in M-P [1998] [all K-values are in millions]:

Fig.1a	$4.5 < K < 8$	and	$0.02 < r < 0.42$
Fig.1b	$9 < K < 13$	and	$0.03 < r < 0.11$

And

Considering the darkest areas in ours [all K-values are in millions]:

Fig. for CPUE13:	$9 < K < 12$	and	$0.02 < r < 0.30$
Fig. for CPUE12:	$7.5 < K < 13+$	and	$0.02 < r < 0.20$

This estimation exercise illustrates or raises a number of points.

1. the Bayesian, subjective analysis in M-P provides estimates similar to those from our GLM estimate and, at least, does not appear to introduce errors.
2. there is a certain robustness in the estimation process in the sense that the CPUE12 and CPUE13 time series are quite different in slope, but they yield similar ranges of parameter estimates.
3. our work could be extended to estimate, e.g., compound AIC-based [Akaike Information Criteria] likelihood confidence intervals, but this seems to be neither the place nor time.
4. the robustness of the results both to the methods of estimation and to the differences in the data sets is a little surprising. It is possible that the model itself has a role that dominates the estimation process, e.g., via its negative feedback non-linear structure, and is at the center of these similar results.

The authors should have carried out similar exploratory data analyses. If they did, the results should be reported in their document. There are also some very important errors in the text in M-P. I assume that they are typos since, if not, they indicate a lack of understanding of Prager [1994]'s equations.

RECOMMENDATIONS

The major problem with production models is that they use almost none of the major amounts of information available about the sharks. A proper stock assessment for the shark complexes would begin with the **demographic, age structure models** that were indeed constructed by the NMFS. The next step would be to move toward the standard **Beverton Holt yield per recruit model** suite. In this context the demographic parameters could be utilized and stock assessment would proceed. Since there are problems with age determination of sharks, **length based stock assessments** should be carried out. There are many references to this subject, only one of which is Gallucci et al. (1996). **Multispecies interactions** of the predator prey type could also be introduced, where the prey are forage food that sharks use. The biomass of the prey species the sharks probably prefer have decreased significantly over the 30 years since record keeping began, leading to a lower carrying capacity. This suggests a further correction that could be made to the production model.

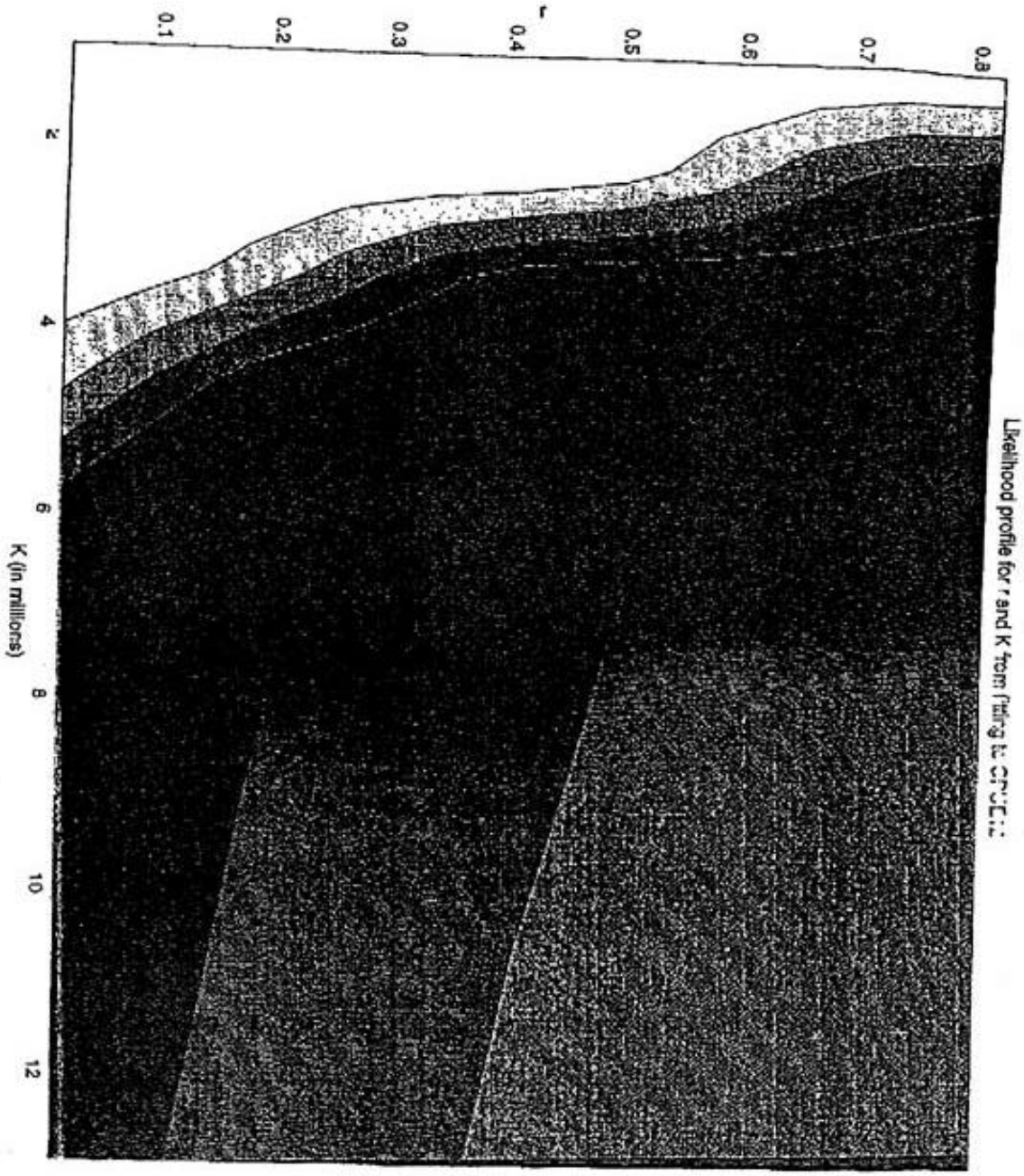
REFERENCES

Gallucci, V., S. Saila, D. Gustafson, B. Rothschild. 1996. Stock Assessment: Quantitative Methods and Applications for Small-Scale Fisheries. CRC Press.

Prager, M. 1994. A suite of extensions to a nonequilibrium surplus-production model. Fishery Bulletin. 92: 374-389.

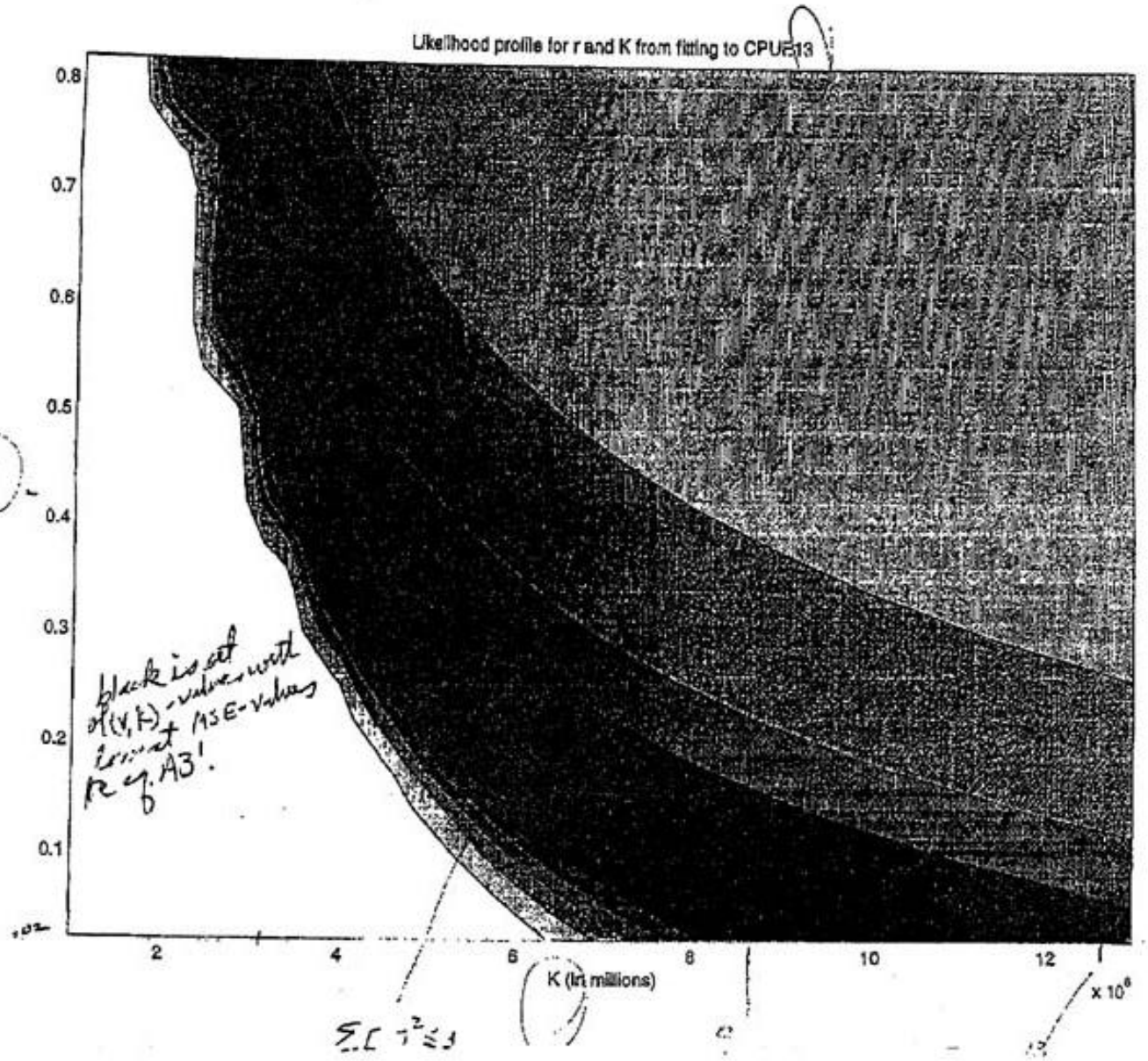
Figure CPUE12 and Figure CPUE13 are attached as well as pages 6 and 7 to this report.

Figure CPUE12



Likelihood profile for r and K from fitting to CPUE12

FIGURE CPUE13



Subject: Fw: supplement to shark report
Date: Wednesday, October 10, 2001 9:02 AM
From: Steve Hughes <shughes@nrccorp.com>
Reply-To: "Steve Hughes" <shughes@nrccorp.com>
To: hollyh@nrccorp.com

> To: Steve Hughes
> From: Reviewer #5
> Re: Supplementary comment to Question # 1.
>
> Question #1:
> Was the model used to estimate large coastal shark population abundance
> and demographic trends reliable and scientifically rigorous?
>
> In addition to what was written in the original report the following
> should be noted:
> Shark life history is quite different than the usual fish life history.
> The usual fish spawns millions of eggs and sperm into the water where
> fertilization occurs. Shark eggs are fertilized internal to the female
> body requiring actual copulation between males and females. The stock
> production model is built around the concept of compensatory dynamics, in
> other words density dependence dynamics where low population density leads
> to a higher per capita growth rate which decreases monotonically as
> population size increases. This is unlikely to be realistic for shark
> population dynamics at low densities. Depensatory models are more
> realistic and more likely to better describe population trajectories.
>
> Steve, please try to transmit as part of your report. However, if it
> presents a problem to do so, my report remains in good shape and will not
> be greatly affected by not including the above. Many thanks.
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